loose it retarded it very much. From this follows the practical truth, that we can produce a very different effect by the same number of clothes according to their make.

"Generally our clothing has been considered as an apparatus for keeping the air from us. This conception is utterly erroneous, and we can bear no garment which does not allow of a continual ventilation of our surface. Just those textures which are most permeable to the air keep us warmest. I have examined different materials for their permeability to air, and taking the permeability of air passing through flannel as 100, linen allowed 58, silk 40, buckskin 58, chamois 51, kid 1 part of air to pass through them. If the above-stated notion were correct, kid would keep us 100 times, chamois warmer by half, than flannel, and so on, while everyone knows that it is quite the reverse."

With reference to Fur the author says :—"A fur is so arranged that its fine hair projecting into the air intercepts all the heat which flows from the surface of the body by radiation and conduction, and distributes this heat through the air which circulates between the single hair-cylinders. Thus the air, however cold it may be, reaches the nerves of our skin as a warmed air. Furred animals in winter, when touched superficially, give a very cold sensation; it is only near the skin that their hair feels warm. In a severe cold, certainly little of our animal heat comes as far as the points of the hair, from which it would escape by radiation or conduction, as the current of air in the fur cools the hair from its points towards its roots, and a severe cold penetrates only a little farther into the fur, without reaching the skin of the same. This can take place only at an exceedingly low temperature, or when a very cold air is in violent motion. In a well-furred animal the changes of temperature in the surrounding air only change the latitudes at the cold and warm zones in the fur; the place where the temperature of the body and the air equalise each other, moves between the roots and points of the hair, and for this reason a furred animal is not warmer in summer than in winter. In summer its heat leaves at the points, in winter near the roots of the hair."

Journal of the Proceedings and Annual Report of the Winchester and Hampshire Scientific and Literary Society, vol. i., part ii. 1871-2 (Winchester: Warren and Son, 1873).

We are glad to see from the Third Annual Report of this Society that it continues prosperous, the number of members being, in 1872, 105. We hope good use will be made of the valuable herbarium of flowering plants, ferns, lichens, &c., collected and arranged by the late Mr. Hill, which has come into the possession of the society, through the generosity of the Mayor, Mr. R. P. Forder, and the President. The present part of the journal contains a number of papers, literary and scientific, read at various meetings of the society. The principal one is the Introductory Address delivered at the commencement of the third session, by the Rev. Canon Kingsley, on "Bio-Geology—the science which treats of the distribution of plants and animals over the globe, and the causes of that distribution." The address is an eloquent one, it can easily be imagined, shows extensive knowledge and great shrewdness, and contains many valuable hints both to young and old naturalists. Most of the other papers are also by clergymen, the principal ones being the following :—"On the Dawn of Thought in Greece," by the Rev. W. Awdry; "On the Metamorphosis of Lepidoptera," by Mr. J. Pamplin; "the Planet Jupiter," by the Rev. E. Firmstone, in which the author gives many interesting facts and speculations as to the condition of that planet; "Vesuvius previous to and during the Eruption of 1872," by the Rev. C. A. Johns, in which the author describes an ascent he made shortly before the last eruption, and appends a condensed abstract of Palmieri's account of the eruption. Appended to the journal is a valuable list of 315 works on the Geology, Mineralogy, and Palæontology of the Hampshire Basin, compiled by Mr. William Whitaker, of the Geological Survey.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Wyville Thomson and the Ventriculidæ

I TRUST that you will afford me a little space for a few remarks upon some passages in Prof. Wyville-Thomson's book, the "Depths of the Sea," which, owing to many engagements, has only just come into my hands. So earnest a labourer in the wide field of truth will not, I hope, deem me discourteous if I point out one corner where his feet have slipped; and if it be objected that, after all, it is only in a small spot, the learned Professor will, I am sure, agree with the answer that, even in the smallest steps towards truth, attainable accuracy is important.

In 1847-48 my father published a series of papers in the "Annals and Magazine of Natural History," which were afterwards collected into a volume, on the "Ventriculidæ of the chalk, their microscopic structure, affinities, and classification." This work, which still remains, I believe, the authority on its subject, introduced order and classification where before all was confusion, expressly founding these upon two guiding principles of anatomy, the existence of which had been proved by searching tests. These two principles—the first being the structure, the second the fold, of the membrane—I am careful to recall, as I think there is considerable misapprehension regarding them. The chief locality of these fossils was in the south and west of England.

In his chapter on the Continuity of the Chalk, Prof. Thomson brings forward several families of ancient fauna as palæolontological evidence in support of his argument. Among these he devotes some attention to the Ventriculidæ (he calls them *Ventriculites*, but why? In the same sentence he uses the family name *Hexactinellidæ*); but, though he acknowledges my father's work, and refers to his "minute and most accurate description of their structure," it does not appear by what follows that he has quite comprehended it: "He (Mr. Toulmin Smith) found them to consist of tubes of extreme tenuity, delicately meshed, and having between them interspaces usually with very regular cubial or octohedral forms" ["Depths," &c p. 482]. This description (the Professor will forgive me for saying so) does not convey a very clear idea of any structure, and certainly does not apply to the Ventriculidæ: if the word "tube" here means the body of the creature, it may in one sense be partially true of a few species in each of the genera—*Ventriculites, Cephalites*, and *Brachiolites;* but if it is intended to apply to the substance of the structure, I must say that it denotes a complete error. My father's words are, that "the membrane of the Ventriculidæ is composed of very delicate fibres," "the fibre is single and solid, never fistular," and that in this structure "*there are no tubes whatever*" (pp. 21, 25, 30). My father carefully describes this membrane, and marks it as the essential characteristic of the whole family of Ventriculidæ. Among the thirty-five species, for the most part marked by strong differences, he points out that *Ventriculites simplex* is the type of the whole family, consisting of a single membrane without a trace of fold.

Now, Prof. Thomson gives a figure of the octohedral structure to which I will not take exception, but he writes underneath it, "*Ventriculites simplex*, Toulmin Smith. Section of the outer wall, showing the structure of the silicious net-work." This implies, while citing my father's name (1) that this structure is proper to that species; and (2) that there is an inner wall. It also speaks of the net-work as silicious, while, two pages before, it is said, that "Mr. Toulmin Smith supposed that the skeleton of the Ventriculite had been originally calcareous." But though mistakes of this sort might easily arise through misapprehension, I must say I was very much astonished to see the figures, one of the entire fossil, the other of the "outer surface," given as "*Ventriculites simplex, Toulmin Smith*," from Mr. Sanderson's collection ("Depths," &c., pp. 483, 484.) A glance at Fig. 1, on the second plate in my father's book, will show that the name

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has been misapplied to this specimen, which, as far as can be judged from the drawing, appears to be either *Ventriculites quincuncialis*, or one of the *Cephalites*, both quite different in outward appearance from the plain *simplex*. I know that it is often not so easy to distinguish the species of those preserved in flint as of those in chalk, but in this instance it is quite evident that it is not *simplex*.

My object in writing the above has been to vindicate my father's scientific accuracy, and to recall the facts he worked out. With regard to another point: it is stated by Prof. Thomson that some of the beautiful sponges discovered in the late deep-sea dredgings, especially the *Holtenia* and its allies, and the Ventriculites, "belong to the same family, in some cases to very nearly allied genera," or, as Dr. Carpenter puts it ("Good Words," October 1872, p. 703):—"Here we found the type of the old Ventriculites, which were supposed to be extinct, still living on in the deep sea." Much as my father would have delighted in the exquisite beauty of these new forms (the *Euplectella* he had examined in 1848), I do not think that he could have acknowledged the *Holtenia* as belonging to the ancient Ventriculidæ; nor, if the use of the word "type" depend for its force upon the character of structure, can it be truly said to be a type of that family. True, it possesses a silicious skeleton, but so does the Euplectella; and neither from Prof. Thomson's description ("Depths," pp. 70-72), nor my own examination, can I discover in the Holtenia any trace of or resemblance to the delicate structure and folded membrane of the Ventriculidæ. With great deference, therefore, to the opinion of these investigators (if I am wrong I will gladly learn), it appears to me that the modern type of the old Ventriculite is yet to be found.

I will add that the series of specimens figured in my father's book is in the Britith Museum, open to examination by students, together with a large portion of his collection of the Ventriculidæ.

Highgate, Sept. 27 LUCY TOULMIN SMITH

"Deidamia"

I NOTICE in Prof. Wyville Thomson's extremely interesting papers the name *Deidamia* v. Willemoes-Suhm, used for a crustacean genus. This name must be changed, inasmuch as it is preoccupied in Articulata by Dr. Clemens in 1859. Dr. Clemens has used the title for a valid genus of North American Sphingidæ. I propose, therefore, for the genus in Crustacea, the name *Willemoesia*, in honour of its discoverer, with the two species *leptodactyla* and *crucifer*, the former the type.

AUG. R. GROTE,
Curator of Articulata, B.S.N.S.

Buffalo, U. S., Sept. 15.

Dr. Sanderson's Experiments and Archebiosis

IN a communication made to the British Association during its recent meeting at Bradford, Dr. Sanderson criticises the experiments of Prof. Huizinga, and also throws doubt upon the validity of the conclusions which I have drawn from experiments of my own. The "Note" appears in your columns this week; and seeing the nature of the conclusion drawn by Dr. Sanderson from his experiments, I am not a little surprised to find no mention in it of one most important point, viz., the temperature at which Bacteria are killed when immersed in fluids.

It must be obvious to all who understand the real nature of the question at issue, that no valid conclusion can be drawn by Dr. Sanderson from his experiments, unless he is able to argue from a definite conviction as to the temperature at which Bacteria are killed in fluids.

Now a study of Dr. Sanderson's writings would show the reader that up to the time of their publication he had every reason to believe that Bacteria were uniformly killed in fluids at a temperature of 100° C. If he still believes this to be true, he cannot (in the light of facts which he has learned concerning the productivity of previously boiled fluids in closed flasks) refuse his assent to my main proposition, viz., that Bacteria are capable of arising in fluids independently of living reproductive or germinal particles.

But the conclusion which Dr. Sanderson does draw from his experiments, and his imputation that facts do not warrant the conclusion of Prof. Huizinga and myself, would seem to imply that he is in possession of some new evidence subversive of his previous opinion, and tending to contradict views which I have recently published concerning the death-point of Bacteria in heated fluids. ("Proceedings of Royal Society," Nos. 143 and 145, 1873.)

As Dr. Sanderson is entirely silent upon this point, I venture to ask, both for my own information and for that of your readers, whether he still believes that Bacteria are killed by a temperature of 100° C. in fluids; and if not, upon what grounds he has changed his opinion?

In the face of his expressed intention (not a little contradicted, as I venture to think, by his public action) of taking no part in the "spontaneous generation" controversy, I ask Dr. Sanderson this question, because I cannot suppose that he would publicly throw doubt upon the validity of the conclusion which Prof. Huizinga and I have drawn from our experiments, in the absence of fresh evidence of his own upon the thermal death-point of Bacteria.

At present he has publicly expressed the opinion that we are not warranted in our conclusions, whilst he has given no sufficient information either to the world of science or to ourselves by which to test the correctness of his own conclusion. This seems neither just to us nor to himself.

H. CHARLTON BASTIAN

University College, Oct. 3

Mr. D. Forbes's Criticism of Mr. R. Mallet's Volcanic Theory

AFTER the lapse of half a year Mr. D. Forbes has recurred in NATURE for Sept. 4, 1873, to my remarks published in NATURE of March 20 last, to his remarks upon my Theory of Volcanic Energy and Heat contained in his review of my translation of Palmieri's "Incendio Vesuviano," which appeared in NATURE of February 6 preceding.

I pray your permission to make some remarks upon Mr. Forbes's last production. They are the last by which I shall prolong this unpleasant controversy.

Mr. Forbes affirmed that if anything was certain, it was that the ejecta of volcanoes in all ages and all over the world are *identical* chemically or mineralogically, and upon this assumption passes a summary condemnation upon my theory, which he predicts will never receive acceptance from anyone—chemist, or mineralogist, or geologist. This rash and I will now say discourteous prediction I at once disposed of by giving the names of two authorities, whose competence even Mr. Forbes could not question, who had already accepted my views.

To this Mr. Forbes now says, that, as these gentlemen possessed for their guidance in assenting to the bare statement of my views, no better information than that upon which he dissented from them, so they may have been mistaken and not he. How is Mr. Forbes sure they had no better information, and can it be possible that he is so dull in weighing the force of evidence as to see no difference in probability of error between two assumed equally competent men—one of whom can assent to a proposition upon his prior knowledge and without waiting for proof; and another, who dissents, before he has heard what can be advanced in favour of the proposition and against his own previous knowledge or supposed knowledge? This, however, is now immaterial except as an indication of Mr. Forbes's capacity for weighing evidence.

To Mr. Forbes's grand objection I replied that it is based upon error as to fact—that it is not true that all volcanic solid ejecta are identical at all times and everywhere.

While I denied, and do again deny, that identity, chemical or mineralogical, exists in those bodies, I admitted that they do present a great general resemblance—which is just what we should expect.

I added a very important remark, namely that whether it were true or false that all volcanic ejecta were identical, chemically or mineralogically—the fact, whether one way or the other, did not apply to or affect my theoretic views as to the nature and origin of volcanic energy and heat; one way or the other, the identity or dissimilarity between the ejecta as found at the surface must be the same, whether they be derived from materials already and constantly in fusion, or be fused by elevation of temperature locally and temporarily produced; the materials fused being the same in both cases.

This last objection, which is fatal to Mr. Forbes's criticism, whether the foundation on which he has rested it be true or false, he either has not noticed or finds it convenient now to ignore.

I illustrated the want of identity, chemical or mineralogical, and yet the great general similarity at all times and places of

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